THE PERCEPTION OF NUMBER.

BY

J. FRANKLIN MESSENGER, M.A.

SUBMITTED IN PARTIAL FULFILMENT OF THE REQUIREMENTS
FOR THE DEGREE OF DOCTOR OF PHILOSOPHY
IN THE
FACULTY OF PHILOSOPHY
COLUMBIA UNIVERSITY

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Introduction.

The experiments upon which this work is based were begun in Harvard University and later were carried on in the laboratory of Columbia University. Part of the results have already been published in the Harvard Psychological Studies, Volume I. It is my purpose in this paper to report some additional experiments and give a more extended theoretical discussion of the questions involved.

The experiments already published had reference entirely to the perception of number through touch. In this report I shall give a brief summary of such of those results as may be useful in the discussion, add a few more from the same field, and then report some experiments on the sense of sight which were intended to throw light on the same general problem.

The investigation began as a study in the fusion of touch sensations when more than two contacts were possible. I had no very definite idea of what 'fusion' meant, and I am even yet unable to find in psychological literature a satisfactory definition or explanation of it. In the absence of uniform usage or common understanding of the term I shall state my own views in regard to fusion in general, in order to make perfectly clear what I mean when I deny that there is fusion of touch sensations when two objects resting near together on the skin are perceived as one object. Inasmuch as the attitude of the subject plays so important a rôle in all of the experiments, it will be necessary also to devote a few paragraphs to the relation of the motor to the sensory elements of consciousness. The conclusions of other experimenters with whom I do not agree also suggest some remarks on that subject.

Fusion.

In Külpe's 'Outlines of Psychology'[1] I find this statement: "If the connected elements are temporally and spatially identical,

[1] English translation by Titchener.

but differ in quality, their connection must be termed fusion; if they differ in duration or extension, colligation."[2] I do not find this distinction useful. If we revolve a black and white disk on a color mixer the elements become temporally and spatially identical. To be sure, there is externally a rapid alternation, but the duration of retinal processes makes the result the same as if the gray were made by actually mixing the pigments. Elements so mixed are sometimes said to 'fuse,' and I presume it would be misleading to deny that meaning to the word. I should prefer, however, a more restricted use of the word, and to call it fusion when two or more qualites are inseparably connected so that one cannot be perceived without the other, *e. g.*, pitch and timbre, color-tone and brightness, and, so far as sight is concerned, color and extension. When two or more elements, which might, under other conditions, be perceived separately, are so connected that their combined effect produces a single quality of sensation, we certainly have a combination of a very different sort, and it is with reluctance that I apply the same term to it. To avoid confusion I should prefer to call it a mixture instead of a fusion. As an example of this, red and yellow mixed give the sensation of orange, which is a single quality of sensation entirely distinct from the other two. With what Külpe calls a colligation I have nothing to do in this connection. A place will be found for that later. It will be noticed that in the above the matter of spatial identity is left entirely out of account. As a matter of fact, two elements which are not spatially and temporally identical usually *can* be perceived separately, and hence do not fuse. It does not follow, however, that they *are* perceived separately; it is sufficient if they *can* be.

Külpe says: "A stimultaneous connection of tones may stand as a typical example of fusion. Colligation occurs, on the other hand, when the cognizability of the separate qualities is either unaffected by combination, so that they retain their original independence, or is actually increased." An important factor, it seems to me, is left out of this classification, viz., the distinction between having a sensation and perceiving an

[2] The German word is *Verknüpfung*.

object. We must also distinguish between qualities which can be conceived separately and those which can produce sensations separately. We can conceive color-tone separate from brightness, but we cannot have a sensation of pure color except when fused with brightness. On the other hand, we can not only conceive each separately but we can also have a simple sensation of red or of yellow or of orange.

It should be noticed in connection with Külpe's statement that 'a simultaneous connection of tones' might be produced by two tuning forks vibrating at the same rate, although their combined effect is a sensation of a single quality. In that case the only way we could recognize fusion would be by finding out the number of objects producing the sensation. However, Külpe's definition of fusion quoted a moment ago excludes such a combination, because they do not differ in quality. But suppose we have one fork vibrating at the rate of two thousand times per second and the other at the rate of two thousand and one times per second. The qualities must be different, and hence the tones fuse; but the difference in the qualities is not perceptible, hence experience could never discover the fusion and we could find it out only by calculating the number of vibrations. According to my classification neither of these cases would be fusion, because either of the two elements could be perceived separately, *i. e.*, neither exists by virtue of the other nor is either in any way dependent upon the other.

Perhaps the question is largely one of nomenclature, but the term fusion has been used in such wide and often vague senses that I think it necessary to state certain distinctions which seem to me to be fundamental. If any one wishes to indicate the distinctions by other terms than those I use I have no objection; but I do insist on the distinctions which my classification emphasizes, however they be designated.

The classification briefly stated is this: There are three kinds of combinations which are fundamental in explaining the relations of consciousness to the external world.

1. Elements may be inseparably connected for perception yet separately conceivable, *e. g.*, pitch and intensity of sound. Neither of these can be perceived without the other, but we can

perceive a variation in one without any variation in the other, hence we come to conceive them separately. This represents the most intimate connection of elements possible and might be called fusion proper.

2. Elements which can be perceived separately may be combined so as to produce an entirely new and distinct quality of sensation, *e. g.*, red and yellow produce orange. In this case a variation in one of the elements produces a variation in the quality which results from the combination of the other two. In this class two qualities combine to produce a third, while in the first class the combination of two elements does not produce a third. It must also be noticed that in the first class each element exists by virtue of the other, and in the second class neither of the original elements entering into the combination owes its existence to the other, but the third quality exists only as the product of the other two. This is a much less stable combination than the former and might be called a mixture. And furthermore, it includes combinations of the first class and thus really represents combinations of a second order.

3. There remains another class of combinations which, for want of a better term, I shall call constructive combinations. By this I mean the combination of different qualities to form a single object. The qualities may be either different qualities of the same sense or of different senses, *e. g.*, we may, by using a single sense, combine weight, hardness, etc., to form an object, or we may, by using two senses, combine color, weight, etc., to form an object. There are many kinds of constructive combinations and one might attempt to classify them, but I am not sure that he could find any very satisfactory basis for such a classification. It will be noticed that this class includes those combinations of sensations of different senses which have sometimes been called 'complications'; but it also includes more than that and it is not determined by the same characteristics.

There is still another way of experiencing the external world, which at first thought might suggest a fourth class of combinations. We sometimes perceive objects in groups, and this grouping might be called a combination of elements into a

whole. I regard this, however, as an altogether different sort of experience, and to describe that experience is the principal object of my present work.

The perception of objects in groups, then, is the question I am studying. The only excuse for saying anything about fusion is that some writers have said that there is a fusion of sensations when two objects are perceived as one. This I have already denied and have given the reasons for so doing. I wish now to take that as a starting point for further discussion, and have given the above scheme for psychological analysis as a means of orientation. My attention has been confined entirely to the senses of touch and sight because the world of external objects is, for the most part, a sight-touch world. Many experiments have been made on the grouping of sounds which are heard in succession, but I am interested only in that which can be given *at once*, *i. e.*, simultaneous stimulations. Sounds of different qualities might be used simultaneously, but I have not tried them.

Experiments on Touch.

I give below a summary of such of the results already published as may be referred to here:

1. Contacts on the back of the fingers are much more easily distinguished when the fingers are spread than when they are close together.

2. If fingers of opposite hands are brought together by interlacing them and two contacts are given on contiguous fingers, they are quite as likely to be perceived as one when the fingers stimulated belong to opposite hands as when they are contiguous fingers of the same hand.

3. There is almost as much tendency to overestimate the number of contacts as to underestimate it.

4. When four contacts are made at a time, one on each finger, certain arrangements are likely to be overestimated while other arrangements are likely to be underestimated.

5. A subject may distinguish a qualitative difference between two points of the æsthesiometer and one point, even though he does not distinguish a difference in number of points.

6. A subject may be led to lose the distinction between the

sensation of one and the sensation of two, and the *Vexirfehler* may be made to appear in a large percentage of cases.

I have attempted to explain these phenomena on the basis of localization and association.

When two exactly similar contacts are given and are perceived as one we cannot be sure whether the subject feels only one of the contacts and does not feel the other at all, or feels both contacts and thinks they are in the same place, which is only another way of saying he feels both as one. It is true that when asked to locate the point he often locates it between the points actually touched, but even this he might do if he felt but one of the points. To test further the matter of errors of localization I made several series of experiments in which I used qualitatively different contacts. This enabled the subject to feel and to report both contacts. For this purpose I took two brass rods about four inches long, sharpened one end and rounded off the other. The subject sat with the palm of his right hand on the back of his left and his fingers interlaced. I stimulated the backs of his fingers on the second phalanges with the sharp end of one rod and the blunt end of the other, and asked him to tell whether the sharp point was to the right or to the left of the other.

The results and observations of the subjects were interesting and very suggestive. I do not think, however, that they can be tabulated in a way to make them significant. They afford, rather, a means of studying the mental habits of the individual. So far as I can see there is no uniformity in the number of errors made, but there were certain types of errors which occurred with all subjects, and individual variations seemed to be fairly well explained by their subjective analyses. When the stimulated fingers were separated by two or more intervening fingers, naturally there was no difficulty in answering correctly, but when the stimulated fingers were adjacent, and often even when one finger intervened, mistakes were made. I usually gave thirty stimulations in a series and allowed only a few moments' rest between each separate stimulation. The percentage of errors is not particularly significant, because some of the contacts were made so far apart that they were judged correctly

as a matter of course. It is the type of errors and the subjects' introspection that are significant. The first thing noticed was that many of the answers were wrong. These errors may be due to any one of three causes. If the subject has no means of determining the direction his answer must be a matter of chance, and sometimes it would be right and sometimes it would be wrong. Chance, no doubt, is the principal factor in many cases. This means, then, that in so far as it is a matter of chance the direction is not really determined. But still the question remains, why can he not tell? When the points are on different hands we must conclude that he has forgotten the relation of his fingers, or that he does not know which fingers are touched, or that he recognizes two qualities of sensation and knows that they are on different fingers and knows the space relations of his fingers but fails to associate the right quality of sensation with the right finger. Any one of these factors would prevent a correct judgment, or all might work together to interfere with it.

There seems to be no evidence that he actually forgets the relation of his fingers, but it is probable that it requires some effort of attention to keep that relation in mind. He has not the relation so firmly established in his mind that he can fit a new experience into it without conscious effort.

There is some evidence to indicate that he does not always know which finger is touched. Mistakes are made in locating. He will often locate a contact on the wrong finger, and often on the wrong hand. This occurs only when two or more contacts are given at once. At least I have never observed it when but a single contact was given. Furthermore, it makes a great difference if the subject is told to notice which hand is touched by each point. If he is judging direction and is unexpectedly asked to name the hand he may not be able to do it. I made some experiments with the same instruments, only asking the subjects to tell if the points were on the same hand, and if not, on which hand was the sharp point. This they could do with scarcely any mistakes. There was some hesitation at first, apparently due to the fact that they had become used to judging direction and it took them a few minutes to adjust themselves to the new experiment, and occasionally there was an answer of

'same finger' when the contacts were on fingers of opposite hands. It will be noticed that in this experiment there was less to be attended to than in the other one. Only two factors enter into these judgments: first, are both hands involved, and second, on which is the sharp point.

The third disturbing factor mentioned above, viz., the failure to associate the right quality of sensation with the right finger, is a matter of some importance, for it undoubtedly does occur and it seeks an explanation. The explanation, I think, is found in the narrowness of consciousness. The recognition of two qualities is a simple matter, and the recognition of the relation of the fingers is not very difficult on account of the long training we have had and the associations that are already formed, but when we are required to hold these in mind and make still other associations between them the process becomes too complex unless it can be done step by step. It cannot always be done step by step, because the sensations fade away too rapidly, even when the stimulating objects are left on as long as the subject wishes. The tendency to move the fingers, which I think has been noticed by all observers, is due to a desire to restimulate in order to notice by itself some doubtful element. I once thought this tendency to move was evidence of the motor element of sensation, but I do not now regard it so. If the subject had control of the stimulating object I think he would just as soon move it, but inasmuch as he cannot do that he moves the only thing he can control, viz., his own fingers. If the operator would repeat each separate stimulation whenever the subject wished, that would answer the same purpose. I postpone further discussion of this question until I have more fully described the experiments.

Besides positively misjudging the direction it was quite common for a subject to say that both points were on the same finger when they really were on different fingers. This was as likely to occur when the points were on fingers of opposite hands as when they were on fingers of the same hand. In fact, I believe that when the hands are in this position there is greater probability of having contacts on corresponding fingers of opposite hands judged on the same finger than there is when the

hand is in normal position and the contacts are made on adjacent fingers of the same hand. I have not made the necessary experiments to speak positively about this, however. This may be because there is less qualitative difference in the sensations of corresponding fingers, or it may be that the fingers of each hand are in their customary relation to one another, while the fingers of one hand are in an unusual relation to the fingers of the other hand. Probably each of these factors has some effect, but I think the error is chiefly due to the latter.

One would naturally expect the reverse error to occur quite often. It did occur, but not so frequently as I expected, considering the success I had had in developing the *Vexirfehler*.[1] I did not make many efforts to induce this error, but in those I did make I was not successful. Instances occurred occasionally in the natural course of the experiments, but I was not able to discover any conditions which would enable me to anticipate errors of that kind. They occurred unexpectedly, just as the *Vexirfehler* did before I learned how to produce it. I believe that with sufficient practice one could learn to arrange his experiments in such ~~a~~ way that the subject would mistake one finger for two in a comparatively large number of cases.

In many cases I was able to anticipate the answer 'same finger' when two fingers were touched. The easiest way to lead a subject to do that was to put one point farther toward the end of the finger than the other. This brought one of them nearly in front of the other and also attracted his attention to a different direction from that he was asked to judge, and, perhaps, rendered it more probable to his mind that they were on the same finger. It often occurred, though, when the points were directly opposite one another. I will give the figures for one subject for one day the first time I tried that method. I first made a series of thirty experiments in the usual way, having the points about opposite each other. In nine cases the judgment of direction was wrong, and once he said the two points were on the same finger. This was rather a high percentage of errors, though not the highest I ever obtained. It was followed by a series of thirty experiments in which one

[1] Reported in *Harvard Psychological Studies.*

point was always farther front than the other. In this series there was one wrong judgment of direction, once he was unable to answer, and seven times he said 'same finger.' So far the contacts had all been made somewhere near the median line of the fingers. On another day with the same subject I made a series of thirty experiments in which some of the contacts were toward the side of the fingers, and one of them always nearer the end of the finger. In this series he answered 'same finger' once when two fingers were touched, twice when the points were on the same finger he failed to recognize that they were both on one finger, and nine times he made errors of direction of the following type: *E. g.*, I put the blunt point on the second finger of the left hand and the sharp point toward the thumb side of the third fingers of the right hand (the right hand was on top of the left and the little fingers outside the index fingers). He answered that the sharp point was to the right, which was correct. I then put the blunt point down again in the same place and the sharp one on the ulnar side of the second finger of the right hand, and he still answered that the sharp point was to the right, which was not correct, but he thought that the sharp point touched the same place both times. The blunt point was in the same place, and he did not notice the changed position of the sharp point. It will be observed that the two places touched by the sharp point are normally near together and were not very far apart during the experiment. A slight variation in the experiments was afforded by asking the subject to clasp his hands so that the second phalanges were even and formed a tolerably continuous surface. The results were of the same sort as in the former cases. If two fingers of the same hand are touched and then one of the points moved to the finger of the other hand lying between them, the subject is likely, in the latter case, to say both points are on one finger. In the first case the contacts were on fingers which are normally adjacent. When one of the points was moved toward the other it was rather natural to have the feeling that it was on the same finger as the other point. Several hundred judgments were obtained from the same subject, but, as said above, the percentage of errors is not significant unless the exact locations of the points are given for

each experiment. So far as I am able to judge, the results mentioned are representative.

With other subjects the results were of the same character. The individual differences seemed to be rather differences in method than in results. One subject took much longer time than the others to make his judgments. He is not a man whose normal reactions are slow. In fact, I should say he is above the average in quickness of thought and action. He seemed to be very careful about his answers and was much disturbed when he thought they were wrong. He made fewer mistakes than the others, but those he did make were of the same sort. He seemed to consider all of the data at his command before answering. When the answer did come it seemed almost as much the product of reasoning as of direct perception. He could more easily and more quickly tell which hand each point was on than he could tell the direction of one from the other. By questioning him in regard to his habits I learned that he does not usually think of things as situated on the right or left of himself, and when he does want to know he has to stop and think which is his right side. When he drilled in a military company he had some trouble learning to start with his left foot, and when he did learn it he did not remember it as *left* but remembered it as *that foot*. It may be that this is about what we all do, but there are, no doubt, differences in our ways of regarding the relation of objects to ourselves. Most persons could judge the direction of the points more quickly than they could judge which hand each point was on. It seems to depend entirely on the habits of the individual.

We must find out the position of the object by means of the part of the body which is stimulated, but we need not be aware of the bodily part at all. In the case of sight we scarcely ever are. If we look at one object with one eye and at another object with the other, we cannot tell directly which object casts its image on the right eye and which on the left. If one object is at the extreme right of the field of vision and another is at the extreme left, the former is seen by the right eye and the latter by the left eye, but the observer cannot discover this except by closing one eye and then the other and noticing the disappear-

ance of the objects. If the eyes were not trained to coördinated movements, and if we were not trained to perceive in a single visual field that which comes to us as two separate sensations, we should be able to recognize directly which objects are seen by the right eye and which by the left. The movements of the hands are sometimes coördinated and sometimes they are not, sometimes two sensations mean two objects and sometimes the same two sensations mean one object. Two fingers may touch a pencil and it feels like one; spread the fingers and with two pencils touch the fingers in exactly the same way that they were touched by the one pencil, and we feel them as two objects, not because the stimulation is different but because we know the fingers are spread and one object could not be in two places. With the eyes we locate objects in relation to one another — one's own body being one of the objects. In so far as we do that with the hands it matters no more which hand is touched than it matters which eye is stimulated by an object of vision. To some extent we do do that with the hands, and some of us do it to a greater extent than others. When one does it he will naturally find it easier to tell the relation of the objects to one another than to his separate hands. If this were the only important function of the hands no doubt we should always be obliged to resort to some unusual procedure in order to tell which hand is stimulated. These two purposes sometimes conflict, and sometimes one and sometimes the other predominates. When a person has adjusted himself to observe the relation of the objects to one another he can judge direction more quickly, and when he has adjusted himself to observe the relation of the objects to his separate fingers he can more quickly judge the hand touched. We see here illustrated two quite different states of mind. The one regards the objects only, and the other regards the objects and the sense organs. The latter is made possible by the power of independent action of the hands.

With all subjects I noticed that it was much easier to recognize the sharp points when the points were not near together. When the points were on near fingers they would often ask to have them pressed harder, saying that they felt two points but could not tell which was the sharp one. When the points were

some distance apart no one ever asked to have them pressed harder, and every one could recognize the sharp point without difficulty. Occasionally I threw in an extra experiment in which I used two sharp points. Sometimes this was noticed and sometimes it was not. If the points were some distance apart it was sure to be noticed. In one series I threw in these extra experiments systematically, beginning with the points on adjacent fingers and gradually increasing the distance. These 'extras' were slipped in along with other experiments to which the subject was accustomed. He did not recognize that he was touched with two sharp points, instead of with one sharp and one blunt one, until four fingers intervened. There were nine stimulations of this sort given without recognition, and with the tenth four fingers intervened and he discovered that both points were sharp. This distance is unusually great. One may reasonably expect two sharp points to be perceived as sharp if two fingers intervene. They may be perceived even when on the same finger, but they are not so likely to be.

I tried a few series of experiments in which the subjects judged whether the sharp point was nearer or farther from the base of the fingers. This seemed very easy to do. Practically no mistakes were made, and no one seemed to have any difficulty in distinguishing which was the sharp point. This might seem contrary to the well-established fact that the so-called space threshold is greater in the longitudinal than in the transverse direction. But in making these contacts there was always a knuckle between the points, and the knuckles form a dividing line and it is easier to tell the difference between two points even on the same finger when separated by a knuckle than to tell the difference between two points on adjacent fingers when not separated by a knuckle.

One of the important factors in determining a judgment of the number of points touching the skin is the direction of the pressure. This in turn is affected by the form of the surface. If two points are placed on a curved surface, they must either press in a different direction or tend to pinch the skin slightly or to draw it slightly. This in part accounts for the smallness of the threshold on the nose. At the end of the nose two

points cannot be far apart without pressing the skin in different directions. Moreover, the threshold in the longitudinal direction is not so very small. It is no doubt smaller than it would be if it were not for the difference in the structure of the underlying tissue. On the middle line of the nose there is not very great power of discrimination in the comparatively homogeneous stretch between the bridge and the soft part. As soon, however, as one point is on the hard bone and the other on soft tissue, they can easily be distinguished by the qualitative difference. And if one of the points presses a little to one side and the other to the other, they pull the skin and are recognized as two. I have noticed when I have acted as subject that two points resting on the nose in a longitudinal line felt as if they were exactly opposite, one on either side of the median line. This was due to a slightly different direction of pressure, one pulling the skin one way and the other the other. In this case they were recognized as two at once, but that does not mean that they were beyond the longitudinal threshold. If they were beyond it they should not have felt as if they were transversely opposite. The transverse threshold is necessarily very small, because it is not possible to have the two points much distant without having a difference in the direction of the pressures. Difference in direction is equivalent to difference in quality of sensation; or perhaps it would be better to say that one sort of qualitative difference is difference in direction. I do not mean that where a certain kind of qualitative difference is present we are able to infer direction. I mean that the two are synonymous and that one is as ultimate a fact as is the other. However brilliant may be the attempts to reduce it to motor or other terms, the same old space relation is there as an ultimate fact when we get through.

Certain physiological processes are, of course, necessary in order that anything be perceived, and it is desirable to find out all we can about those processes, but nothing is gained by trying to substitute the process for the object of perception. The movements employed in *measuring* space are themselves objects of perception and hence may serve as a basis for comparisons. We may measure a room with a yard-stick, but we

do not reduce the room to a yard-stick. We use a stick because it is easy to carry about and serves as a means of comparison of different distances. In the absence of a stick we may step it off and thus make comparisons by means of movements. This comparison forms the basis for associations and thus aids perception. The movement of stepping off distances or of running the eyes or fingers over them is for purposes of comparison and description, and it aids perception in the same way that writing out a thought helps one to comprehend it more completely. Writing out a thought fixates the associations composing it, measuring a room with a yard-stick fixates its size, measuring it with eye movements does the same thing, only less accurately and perhaps less permanently. We would use sticks more and eye movements less if sticks were always available and as easily handled as are eye muscles.

In all of my experiments the actual space relation has been one of the chief elements of perception. It appears in two forms, distance and direction. Each supplements the other. When the distance is small the difference in direction must be greater, and when the difference in direction is slight the distance must be greater in order that two points can be perceived separately. The threshold for two points is not a space threshold, but rather a space-direction threshold. But even this is not sufficient to account for the perception of two things. So far as the immediate excitation is concerned they are the determining factors, but number is not given immediately. Associations must be formed before the idea of number arises. The formation of these associations is a very complicated process and is possible only to an intelligence of a very high order. Some writers conclude that birds can count because they notice it when eggs are taken from the nest. A bird may well know the difference between two eggs and four eggs without knowing anything about the *number* of eggs in either case. A bird may know the difference between a worm with twenty legs and a worm with none, but who would say that that is evidence that the bird knows how many legs the one has or that it has a *number* of legs at all? It happens that we distinguish the worms by other characteristics than the number of legs, while we dis-

tinguish the nests only by the difference in number of eggs. In this particular case the most convenient way of recognizing the difference is to note the number of eggs. This is the only way we can describe the difference, and the necessities of description have much to do with determining the perception. But to a bird the difference between a nest of two eggs and one of four may be as great as the difference between a bald head and a bushy one is to us. We could easily conceive a being to whom the only difference between a bald head and a bushy one was a difference in number of hairs. In fact, we might describe the difference in terms of number, but we do not perceive it that way. We describe a great many things in terms of number which we do not perceive that way. We may describe a square as having four sides and a circle as having an infinite number of sides, but no one needs to know the number of either in order to recognize the difference. I shall have more to say about the number concept on a later page. It is sufficient for the present if we bear in mind our inclination to read into perception attributes which we have found convenient for purposes of description.

I shall pass now to a consideration of the sense of sight and shall report the experiments made on that sense.

Introduction to Experiments on Vision.

Naturally, we should not expect to find exactly the same phenomena in connection with sight that we find in touch, for sight is a more highly developed sense. This necessitates different methods of experimenting, though it does not, at least *a priori*, necessitate different principles of explanation. There are two important differences between the organs of sight and those of touch. First, eye movements are always coördinated, while movements of the hands are coördinated only to a very slight degree. The two eyes always sense the same objects in the main part of the field of vision, the two hands may or may not. If we moved each eye independently of the other, we should have to adjust ourselves to objects in a very different manner from our present one. I have said that many errors of touch are errors of localization. I have said that one thing that seems to occupy two places is called two, and that two

things which seem to occupy one place are called one. This is very familiar in the case of sight. Two pictures brought to the same place by a stereoscope look like one, and one picture seen in two places looks like two. If we train ourselves, as we easily can, to look with parallel vision, we see double when we look at one object, and we may seem to see but one when we look at two if they are properly placed. In order to see two things as one they must be just alike or nearly so, for otherwise there would be retinal rivalry; but this retinal rivalry is due to the fact that we know two things cannot occupy the same place at the same time. We cannot perceive a thing in a way that would be altogether impossible in nature. We may wear glasses which make things appear inverted, but there is nothing impossible in that. We see things inverted every time we look at the reflection in water. Retinal rivalry finds its parallel in touch when a sharp and a blunt object on the hand seem to be in the same place, only we can conceive a sharp point surrounded by a larger object, and in my experiments I found this was often done. In that case the rivalry disappeared, just as it does in stereoscopic vision when one picture is of such a size and nature that it can be conceived as existing within the other without destroying either picture.

A second difference is that the organs of sight are in constant motion, while the organs of touch may be kept still. Inasmuch as the hands are the most used organs of touch and my experiments were all made on them, I speak of them as if they were the only organs of touch, and for present purposes I shall consider that we feel with two hands as we see with two eyes and, for the most part, disregard the rest of the body. In our ordinary experiences we do not often realize how much depends on the constant movement of the eyes, but the moment we begin to experiment we must take it into account. It is comparatively easy for a subject to sit down and hold his hand still while the operator touches him with something, but it is impossible for him to keep his eyes still when something is shown him. They will move unless some extremely artificial means are used to prevent it. Such artificial means are not only inconvenient, but interfere so much with normal vision that they are undesir-

able. The only practicable way seems to be to make the exposure too short to allow much eye movement. This makes it necessary that the subject base his judgments on memory and not on direct perception. He has not time enough to form a judgment until after the stimulation has ceased. The sensation may endure for a short time after the light is shut off, but not so long as it usually takes to form a judgment. The after image may sometimes serve as a guide, but few persons are sufficiently well trained in observing after images to get any help from that source, and if they did it would not be exactly the same as a direct perception. Therefore, it seems evident that in the case of sight the judgment depends largely on memory. But so it does with touch. The stimulating objects may be left on the skin for a long time, but the subject names the most prominent contacts first and remembers them while he looks for others. He does not perceive them all at once as separate individuals. He has one advantage in touch in that he can go back over the field and verify his count by direct perception. He cannot do this with sight because the objects are no longer present. He cannot always do it with touch, for often some of the sensations fade away before he gets to them a second time.

The power of rapid and constant movement of the eyes is undoubtedly one of the causes of the high development of the sense of sight. The movement of the fingers over an object in feeling it corresponds to the movement of the eyes in looking at it. We are very familiar with the acuteness of the sense of touch when the fingers are moved over a surface. If a subject is allowed to move his fingers even a very little he can often tell how many contacts are on them, when he cannot tell without moving them. There is the same tendency to move in the fingers that there is in the eyes, but not to so great a degree. It requires special effort for some persons to hold the fingers still when they are trying to observe the number of contacts on them, but they can be held still more successfully than can the eyes. If the eyes are held even approximately still for a short time the objects before them blur. This applies when the objects themselves are stationary. Sensations of touch readily blur or fade if neither the hands nor the objects touching them

move. It may be true that we do not see objects when the eyes are in motion, but it is also true that we could not see objects well if the eyes remained stationary, unless the objects moved. Rapid alternation of movements and pauses seems to be the condition best fitted for seeing. The same is true of the fingers in feeling. When we want to feel a surface we move the fingers back and forth over it. This alternation produces repeated stimulations. Subjects get help often by pulling on the muscles of a finger even though there is no perceptible movement of the finger, and sometimes even when I could not detect any finger movement and the subject did not realize that he was using any of his muscles I have been able to see a tightening of the cords along the back of the hand when he was making special effort to perceive contacts on his fingers. This may help to direct the attention, and it certainly restimulates the part to some extent. This tendency to move in order to make sensations more distinct is conspicuous in sight, touch and taste, and is also present to some extent in smell and hearing, though in the two last-mentioned senses it is more noticeable in animals than in men. All of these movements increase the excitations of the end organs, and they may increase the sensitivity by increasing the blood supply. It may be that the remarkable sensitivity of the skin in the blind, and especially in the blind and deaf, is due to an acquired power of delicate movements, for delicate movements must make possible delicate and finely differentiated excitations.

A number of recent writers have laid great emphasis on the motor element in sensation, and the opinion is somewhat widespread that there can be no sensation without a response, and that the response determines the sensation and is a part of it. I have myself been an enthusiastic supporter of the view that an efferent process is as essential to sensation as the afferent process, and for some time I thought my own experiments confirmed that view, but after more careful consideration it seems to me that it is inadequate and rests on an incomplete consideration of the facts. In a recently published work on 'The Practice Curve'[1] Dr. J. H. Bair has taken that point of view

[1] PSYCHOLOGICAL REVIEW, Monograph Supplement, No. 19, November, 1902.

and has interpreted his results accordingly. The next few pages will be in part a criticism of his position.

If we imagine a bird soaring in circles above our heads, we no doubt find on introspection that the eyes tend to move in circles in much the same way as they would if we actually saw the bird. It might be argued from this and many other similar experiences that the idea and the movement are inseparably connected. If they are, what is the connection? Does one not think of the soaring bird before he moves his eyes? If I close my eyes and think of an object at my right I find my eyes tend to turn to the right, and then if I think of an object at my left I find my eyes tend to turn to the left. But why do they turn to the left, unless it is because I have first thought of something *there?* If, instead of thinking of an attractive or indifferent object, I think of a repulsive one, the eyes tend to turn away. Why should the eyes turn one way in one case and the other way in the other, unless some conception of the nature of the object is present in the mind before the movement takes place? If I know that my hat is on the table at my right and some one says 'hat' to me I tend to look or move toward the hat, but certainly there is nothing in the sound of the word or in the idea of the thing that could lead to movement in any particular direction. It could be nothing other than the consciousness of the position of the hat, and this consciousness must have preceded the movement. It is here an altogether accidental attribute of the hat which determines the response. I may never have reacted in that way to 'hat' before. The word in this case suggests position, and the position suggests the movement. The reaction is not called forth by the thing but by the space relation.

We sometimes say one cannot have an idea of a thing without having some idea of what he would do with it, or, more technically, without knowing how he would react toward it. The idea of cutting is inseparably connected with that of knife, but so is the idea of patent medicine inseparably connected with the Brooklyn Bridge. Cutting usually accompanies the knife and, unfortunately, patent medicine bill boards always accompany the bridge. The association of bodily movements

with mental states is no more intimate nor essential than association of external objects. It is no more significant in an explanation of conscious processes. The only difference between the two is this: We can control bodily movements and we cannot always control external objects. We try to reproduce situations when we think of them. If we imagine ourselves riding on horseback, we tend to reproduce the movements we have made while riding, because we *can* do that. If we could reproduce the horse as easily, no doubt we should do it, and then psychologists would have good grounds for concluding that one could not think of horseback riding without having a horse between his legs, or at least tending to have one there.

Mr. Bair, in the work referred to above, gives the following incident in support of his position. He says: [1] "The fact that the bodily adjustment, even though arbitrarily made, recalls to mind the feeling and experience usually connected with that adjustment is very prettily illustrated by a two-year-old child whose associations I have studied. Last summer her father took her with him fishing, and she was taught to show how big a 'fishy' papa caught by spreading her arms. One day six months later, when for a long time she had not shown any one how big the fish was, her father had her standing on his knee, when all of a sudden she lost her balance and threw out her arms to regain it. This adjustment called to her mind her former association and she exclaimed, 'O, papa, fishy, fishy!'"

This is an interesting example of childish association, but the fact that it was an attitude of the body which called forth the idea of 'fishy' is no more significant than if anything else had suggested it. Bodily movement is an object of perception as much as is a fish-pole, and either may suggest a fish, and the method of association is the same in either case. Nor do I regard it as significant that it was her own bodily attitude which suggested the fish. Some movement of her father might have served as well and in the same way called up the association. The body is a part of one's environment and as such affects us. If I draw down the corners of my mouth and look melancholy it tends to make me feel melancholy. But if I see

[1] Page 56.

another man looking that way that also tends to make me feel melancholy. Wherein lies the difference? In just this: I can raise the corners of *my* mouth if I try, and I cannot raise *his* by direct effort of the will. I may be able to affect him somewhat, but not so easily or quickly as I can my own body. Moreover, my own body is a *constant* element of my environment, and the matter of constancy has much to do with the amount of influence an object has. The reason we think of objects as located in relation to our own bodies is that our bodies are always present. If a post were always in a conspicuous place in every group of objects we perceive it would serve the same purpose just as well, and it would be a law of perception that things must be regarded as lying on one side or another of that post. If I see a stranger with the signs of sadness in his face the effect on me is comparatively slight, but the same signs seen in the face of a friend have a much greater influence merely because the friend has been more often a factor in my environment than has the stranger, and because I am more attached to him. Just so, I am still more attached to my own body because it is always present and has grown dear to me on account of associations. Sometimes, indeed, one can change his external environment more quickly than he can his bodily adjustments and mental equipment. Cases of sudden conversion often illustrate this. A man becomes deeply convicted of sin and is converted, and, having religious emotions in his soul but only campaign expressions in his mind, in a moment of ecstacy shouts out 'Hurrah for Jesus.' His motor response was not at all the correlate of his state of consciousness. He had the proper emotions, but the proper corresponding motor element was entirely lacking because he had not formed the necessary associations. Having reacted in the wrong way his reaction may become an object of thought, and influence his next thought and his next movement; but if he had thrown up his arms and knocked down a chandelier, the falling of the chandelier would also influence his next thought and his next reaction, and the question again comes forward, How does the influence of motor reactions differ from the influence of external events?

Last year I collected a large number of answers to the question, How many one-dollar bills will equal in weight a five-dollar gold piece?[1] I found that people are altogether unable to form a judgment. The average estimate was 2,291 bills and the median estimate was 45. The actual number required is a little less than seven. I intended by that question to show that we cannot form judgments about qualities toward which we have never consciously reacted. I would not now state it that way. The motor response to money has been exactly the same as it would have been if weight determined the value of both bills and gold, but we have formed different associations. We have not consciously reacted upon the weight of bills; but that word 'consciously' simply implies associations and does not imply any difference in the reaction.

A psychologist once told me that he could not form a mental picture of an elephant and at the same time put his vocal organs in the position to say dog. He gave this as an example of thinking in motor terms, or thought conditioned by motor adjustment. But the conclusion does not necessarily follow from the fact. Certainly it is just as logical and it seems to me more consistent with other facts to say that it means that he cannot think of dog and elephant at the same time. For, in order to make the motor adjustment necessary to pronounce the word dog he must think of dog, and that prevents him from thinking of elephant. In matters of speech, as well as other voluntary movements, we do not will the movement directly but will the result. The result in this case is the word dog, hence that word must be in mind, and the narrowness of consciousness may explain why one cannot consciously pronounce one word while thinking of another. We often do by a 'slip of the tongue' speak one word while thinking of another, but in that case the spoken word is not willed. The limitation is in the will and not in the vocal organs, because we cannot will an act without thinking of it.

In just the same way I should explain the fact, mentioned by Mr. Bair in his thesis, that people cannot repeat the words *one one one* and think *a b c*. When a person thinks *a b c* he

[1] Published in *Science*, April 25, 1902.

cannot think *one one one*, and hence cannot pronounce the words. There is an abundance of evidence to show that one cannot attend to two things at once unless they are related to each other so as to form some sort of a unit. We might represent visually the letter *a* and the figure 1 together, we may have seen them together. But we cannot pronounce the word *one* and the letter *a* at the same time, hence we have never associated them in simultaneous connection and cannot group them together, or in other words think of them as any sort of a unit.

We may grant that there is no thought without expression, or at least that thoughts tend to be expressed, but it does not follow that the expression is one of the conditions by which thought exists. Accidental attributes of a thing usually determine the response. Hat-on-the-floor calls forth a different response from hat-on-the-rack. The mere accidental position has little to do with the sensation produced by the hat and nothing whatever to do with its nature or function. Yet it is the position which determines the response. If it is on the floor, it is the incompatibility of its location and its function which determines the response. It is not thinking of a thing that leads to action, but thinking where the thing is. It is not so much thinking what we shall do with it as where we shall get it. If I close my eyes and think intently of a pencil on my desk I notice an inclination to reach for it, but I do not notice that thinking of the pencil makes me inclined to write with it, although pencils are made to write with, not to reach for, and I have spent more time writing with them than reaching for them. If I think intently of a knife lying beside the pencil I also feel impelled to reach for it, and I can discover no difference in the two responses, though the ideas of the two things are altogether different. I feel no more inclined to whittle with the knife than to write with the pencil. The thought of knife and pencil never impels me to sharpen the pencil and write. It is only the thought of something I wish to say that impels me to write. Bodily adjustments may help or hinder the flow of thought, but that has nothing to do with the response to the thought when it has once come. Mr. Bair says: "A person who is able to produce good compositions when writing may find difficulty in

thinking when writing on a typewriter," and regards that as evidence that "all our thoughts are accompanied by movements and are in turn conditioned by movements." It is quite true that one who is not accustomed to composing on a typewriter cannot do it easily. It is quite probable that many scientific men can write letters easily on a typewriter, but could not write a scientific article on one. Does that mean that they think their scientific thoughts in terms of pencil movements and their correspondence in terms of typewriter keys? I cannot conceive it that way. I find myself unable to do useful thinking while riding a wheel, but walking does not interfere with my thoughts. I do not, however, believe that this shows that my scientific meditations are conditioned by the steps I take and that the aimless reverie is conditioned by the movement of pedaling a wheel. It is only that I have made it a practice not to think much while riding, but when walking I do study. A student sometimes forms the habit of studying a certain subject in a particular room and is distracted if the furniture is rearranged or he is compelled to study that subject elsewhere. Our thoughts are as much conditioned by external environment as by bodily adjustment. I do not deny the effect of the motor system upon consciousness, but I maintain that its effect is due to the fact that a reaction is itself an object for sensation, rather than an element of sensation, and produces its effect in the same way that external objects do.

If a motor process is an essential element of sensation I cannot account for the fact that we can feel the movement of an object vibrating more rapidly than it would be possible to move the muscles. We feel each separate vibration but we react only to the series of vibrations and not to each separate one.

Another difficulty, which for me is insurmountable, is how it is possible to perceive a reflex movement itself. On this theory a sensation is the result of an afferent and an efferent nervous process. Then, in order to have a sensation of a reflex movement of one's own hand, it would be necessary first for an incoming current from the hand to reach some center and be reflected so as to produce a motor response. This response may not involve the muscles of the hand. It may be simple or

highly complex, but there must be a response. Then only would the sensation arise, but this sensation must also have some response, for otherwise it would not be a sensation, and the response must be made before there is a sensation. What was said of the first response may be said of the second and so on *ad infinitum*, and no sensation of a reflex movement can arise until the last member of an infinite series is reached, and considering the slowness of nerve processes it would take a very long time to have a real sensation of a movement if it had to come that way.

Experiments on Vision.

The apparatus used for the experiments on vision was the ordinary drop-screen. It was held up by a magnet, and the subject released it himself by changing the direction of the current with a pole changer. He was thus able to make the exposure exactly at the moment of maximum attention. Each one chose his own distance from the screen — usually from 18 to 30 inches. The opening in the screen was wide enough to make an exposure of one one-hundredth of a second. A white card three centimeters wide was placed behind it, and on this card were drawn lines or dots and the subject was asked to judge the number. The figures reproduced (see Plate I. at end) show approximately the size of the lines used. Heavier lines are perceived more readily and accurately, and lighter ones are perceived less accurately. Each card was exposed three times in succession, thus giving the subject an opportunity to correct his first statement if he chose. He recorded his judgment of the number every time the card was shown, and in working out the results I took the sum of the three. Space was left on the paper for him to draw the objects as he saw them. He did this partly as an aid to himself and partly as suggestions for the experimenter. He was not required to draw them every time, but was free to do whatever was most helpful to him. Most subjects drew the figures quite often, especially when something new was shown them. They often found it necessary to make the marks on paper as they appeared to them and then to count them before assigning a number.

The first set of experiments was planned for the purpose of showing that increase of complexity does not imply increase in difficulty of judging, or it would be more in accord with my point of view to say that increasing the number of parts does not, of itself, increase the complexity of the sensation produced by it. This was very evident in the sense of touch.[1] For this purpose figures B, C, D, F and G were made. There were five cards in each series, the only difference in the cards of a single series being a difference in the distance between the lines. The lines were three millimeters long and the space between them was 2 mm. on the first card, 4 mm. on the second, and so on, up to 10 mm. on the fifth card. For this particular experiment there was really nothing gained by having so many cards of the same kind, as the variation in distance made no difference. But before I began I thought perhaps the distance might have something to do with the judgment. Even at the greatest distance the lines were within the field of distinct vision, and perhaps one should not expect any variation in perception. The fact that there is no difference, however, will be significant when compared with some results which will be given on a later page. Figure B is made up of the smallest number of lines, and hence is the simplest. Figure C has twice the number of lines, making it just twice as complex. It consists of a pair of lines just like figure B, plus another pair of the same kind, except drawn in the vertical instead of the horizontal direction. If small numbers are easier to perceive than larger ones, then B should be perceived more easily than C; but if we find that adding two lines does not make it more difficult to perceive, or if it makes it easier to perceive, then an explanation must be based on some other factor than difference in complexity. If C is more easily perceived than B, it may be because the addition of two lines makes it blacker and hence more visible, just as several small dots are visible at a distance when only two or three of them by themselves could not be seen at all. This matter can be tested by the use of figure D. It is just as black as figure C. It consists of the same number of lines running in the same direction but not bearing the same

[1] *Harvard Psychological Studies*, Vol. I., page 139.

relation to one another, hence the figure is quite different. If C is more easily perceived than B on account of being blacker, D should be perceived just as easily as C, for it is just as black. If vertical lines are more easily perceived than horizontal, or if lines running in two directions are more easily perceived than lines running in one direction, C might be more easily seen than B, but there could be no difference between C and D on that basis.

TABLE I.

	Ba	*Ca*	*M*	*Fa*	*H*	*G*	Av.
B	50	70	35	34	54	50	49
C	5	37	16	20	5	13	16
D	9	69	52	72	50	27	46

The accompanying Table I. contains the results obtained from figures B, C and D in terms of percentage of error. The vertical columns show the average errors for six subjects, and the horizontal rows afford a comparison of the three figures. The last column contains the final averages for each figure. There is a slight inaccuracy in the results, brought about by the fact that some of the errors were errors of overestimation and some were errors of underestimation. In adding up the results for each individual subject an overestimation sometimes offset an underestimation, but I do not think this occurred often enough to make it necessary to complicate the table by showing these variations. Figure D was overestimated only once, figure C but few times, figure B more often. One subject, given in the table as *Ba*, overestimated B very often, so that his actual errors were more than the 50 per cent. attributed to him in the table. But since the final average is an average of individual averages, these cases do not affect it to any great extent. It may make a difference of one or two per cent. in the average for figure B, which would not change the significance of the results.

It will be noticed that the errors for B and D are about three times as great as for C. All subjects said that figure C was much easier to perceive than either of the others, and the tabulated results agree fully with their subjective reports. There was much less feeling of uncertainty and much less hesitation

in answering when series C was given. A comparison of the number of errors in series B with those in series D shows approximately no difference. We may then rule out those factors mentioned above, blackness and direction of lines, as having nothing to do with the judgment of number, and we must conclude that the form of the figure as a whole is the important factor.

A comparison of figures F and G will throw some light on the question of form. Here there are two things to consider, the amount of error and the nature of it. Instead of giving the percentage of errors I will give the total number of lines perceived by each subject. No subject except *Ba* saw either of these series more than once. There were five cards in each series and each card was shown three times, but when the whole set had once been given it was not repeated at another sitting. *Ba* was given the same thing on four different days. In his case I give the average for the four days. It will be noticed that the actual number of lines seen in series F is 75 and the actual number seen in series G is 120. Table II. contains the total number of lines reported by each subject.

TABLE II.

	Ba	*K*	*Ca*	*M*	*H*	*G*	*F*	*A*	*S*	Av.	Av. Error.	Per cent. Perceived.
F	94	144	49	53	114	72	45	108	98	86.3	29	1.15
G	119	149	70	94	118	105	112	112	111	110	17	.91

The first thing observed is that the average estimate of the number of lines in series F is above the actual number, while in series G the average estimate is below the actual number; and the difference between the average estimate and the actual number is greater for series F than for series G. In the former the total number of lines reported by the subjects is 115 per cent. of the number given, and in the latter the total number of lines reported is 91 per cent. of the number given. More significant still is the average error, which for series F is 29 and for series G is 17. Not only is the average error less for series G, but the direction of the error is practically uni-

form. Only one subject overestimates series G. All the others underestimate it with some degree of uniformity. Six of the remaining eight are quite uniform, and only one, *Ca*, is greatly below the rest. In series F five overestimate and four underestimate, and there is scarcely any uniformity. The mean variation from the average error is practically the same foı both. It was not really necessary to carry out these figures; a glance at the table is sufficient. Uncertainty and variability characterize the judgments in series F, while much greater accuracy and more uniformity are found in series G. The reason is not hard to see. The lines in figure G make a complete figure. Figure F is the same thing with three lines left off. It would be a complete and symmetrical figure if turned around so that the two lines running in the same direction formed a base, but as it stands it suggests something like figure G but yet there is something lacking and it is hard for the subject to discover what is wrong. At one time it looks like one thing and at another time like something else, and he cannot make out what it is. As mentioned above, the variation in the distance of the lines from the center had no effect on perception. The same card might just as well have been given fifteen times in succession except that the subjects knew that the cards were changed and hence knew that there might be a difference. It will be seen that subjects *K*, *H* and *A* saw no appreciable difference between the two series. They seemed to get the idea of a group of lines arranged about as they are in G, and they would often draw the figure as they seemed to see it and then count the lines. Some subjects noticed that the figure was incomplete and would make some allowance for the gap at the lower left-hand corner, but they could not tell how much allowance to make. Still others were unable to get an idea of any definite figure out of it. They knew there were a few lines, but were always very uncertain of the number. They were quite sure to underestimate. We can easily see why subjects tend either to overestimate considerably or to underestimate considerably. Some are quite ready to fill out the missing places and think they see them. They give their attention chiefly to the part they actually see and supply the rest, just as

we habitually supply missing parts in the field of vision. Others notice the gap, and in their efforts to make out with certainty what is there and what is not, give more attention to the missing part, and being careful not to report any more than they are sure are there, fail to see all, and underestimate the number.

With series G there was much less feeling of uncertainty. It was not unusual for a person to regard the figure as consisting of six lines instead of eight, in which case it would be symmetrical and complete and in form much as it actually is. Occasionally only part of it seemed to be seen, and it would be drawn in a way more or less similar to figure F. It makes a great difference what the subject attends to. He attends to the thing as a whole if it has enough unity to enable him to do it. If it has not or if he fails to grasp it as a unit, he must attend to one part of it and infer that the other part is what he would naturally expect it to be, or he must attend to one part, and refusing to trust to inference for the other, report what he does see and make no account of the other part.

One might say that it is easier to perceive a number of objects when arranged with regularity than it is when they are not so arranged. That is true to some extent and under some circumstances, but one could arrange a set of experiments which would seem to disprove it, or perhaps to prove the opposite. I made a few experiments bearing on this question. For this I used dots, from 5 to 20 in a group, instead of lines, and compared the judgments of the number of dots regularly arranged with the judgments of the same number irregularly arranged. The observers felt less certainty when the dots were in irregular order, and made a greater number of errors. About all they could do was to get some notion of the quantity and then reason out how many it would take to make up such a quantity. This they could do surprisingly well but, of course, they were likely to make slight errors the most of the time. When the dots were in regular order they did not make so many mistakes, but when they did they were quite likely to make greater ones. For example, nine dots in irregular order were never judged to be more than twelve, but the same number of dots arranged in three rows of three dots each was called sixteen. The observer recog-

nized a square group of dots, but he thought it consisted of four rows with four dots in each row. This made him estimate the number as nearly twice what it actually was.

In all there were 186 judgments given by four subjects. One half of these judgments were of regular and the other half of irregular groups. Counting up the total *number* of errors made, I find that out of the 93 judgments of regular groups there were 42 erroneous answers, and that out of the 93 judgments of irregular groups there were 51 erroneous answers. But when I figure up the total *amount* of error I find that the 42 wrong answers about regular groups contain a total error of 160 dots, or an average of 3.8 dots per wrong answer; and 51 wrong answers about irregular groups contain a total error of 149 dots, or an average of 2.9 dots per wrong answer. In other words, the number of errors bears the ratio of 4 to 5 approximately, and the amount of error the ratio of 4 to 3.

I shall make further use of this experiment in the conclusions which will follow the experiments. For the present it may help to show that the difference between series F and series G is not due to the fact that one figure is regular and the other, though in reality regular, does not give the impression of regularity. The difficulty of judging the number of objects in a group must involve other factors than the number of objects and the regularity of their arrangement.

It will be useful to know if a number of objects bunched close together is perceived with greater or less difficulty than is the same number when scattered over more surface. I prepared four sets of cards bearing respectively 8, 9, 12 and 15 dots of about one millimeter in diameter. Each set contained three cards. On one the dots were very close together, on another they were scattered as much as the apparatus would permit, and the third card was a medium between the two, as nearly as I could make it. No accurate measurement of distance was possible, as no two were alike and I tried to arrange the dots as irregularly as possible. Each card was shown three times in succession, as in the previous experiments; thus each observer made 36 judgments. Table III. contains the results for five subjects. The figures represent the number of errors made.

The amount of the error in each case is not taken into account. The direction of the error is indicated by the sign before each figure : — means underestimation, + means overestimation. C, M, and S at the top of the columns mean close, medium and scattered, respectively.

TABLE III.

	C.	M.	S.
Mi	+ 1 — 11	+ 1 — 8	+ 3 — 4
M	+ 1 — 6	+ 9 — 0	+ 5 — 1
A	+ 0 — 10	+ 5 — 0	+ 6 — 0
Ba	+ 1 — 7	+ 6 — 2	+ 12 — 0
J	+ 7 — 4	+ 7 — 1	+ 10 — 2
Total.	+ 10 — 38	+ 28 — 11	+ 36 — 7
Total Errors.	48	39	43

It will be seen at once that the number of errors is approximately the same however the dots are distributed. The difference between 48, 39 and 43 is not enough to be considered. The subjects themselves seemed to regard one kind of distribution as about as easy to perceive as another. The significant fact is that the errors of underestimation decrease as the dots become more scattered and the errors of overestimation increase. One increases just about as rapidly as the other decreases. One might expect that there would be fewer errors in the medium group than in either of the others. If instead of counting the number of errors we take the total number of dots shown and compare with the total number reported by the subjects, the underestimations would offset the overestimations, and the remaining error would be comparatively slight. I made this calculation and found that the algebraic sums of the errors were as follows: Close — 67, medium + 39, scattered + 71. The total number of dots shown in each group was 660. When they were close together they were reported as 593, when they were somewhat scattered they were reported as 699, when further

scattered they were reported as 731. These figures are the totals for the five subjects.

If we could say, as these results seem to indicate, that the closer objects are together the fewer they appear to be, and that they appear to increase in number in proportion to the increase in distance between them, we should say that I did not succeed in making the medium group a real medium, but that the dots were scattered a little more than they should be. It would have been better, probably, if the dots in the medium group had not been scattered quite so much. It is going too far, however, to say that the apparent number increases in proportion to the distance. It does in a general way under certain conditions. It is a common practice to spread things out when we want to make them appear more numerous than they are. Men marching in a procession scatter out when they wish to appear like a great number. But if distance always produced this effect, lines arranged as they are in figure G should appear more when they are 10 mm. apart than when they are 2 mm. apart. Distance is an important factor when working by itself. It may or may not be an important factor when in company with other factors which may outweigh it.

In order to test further the influence of distribution and at the same time discover whether the size of the objects had any influence upon the judgments of number, a new series of cards was prepared. There were nine cards, each containing fifteen spots. The spots were of three sizes and arranged according to three different methods of distribution. The first card had on it small spots close together, the next small spots slightly scattered, and the third small spots still more scattered. The next three cards contained spots which were a little larger and arranged according to the same order of distribution. The last three contained still larger spots in the same order. Aside from the matter of distance between the spots regularity was avoided as far as possible. Figure O is an exact reproduction of three of the cards, showing the three sizes of spots and the three different distances apart. No. 1 represents the smallest spots close together, No. 5 the middle size spots at the medium distance apart and No. 9 the largest spots at the greatest distance.

Each of the nine cards was shown three times by the same method used in the previous experiments. The order in which the cards were shown was (1) close together, *a* small, *b* middle size, *c* large; (2) medium distribution, in the same order of size; (3) scattered, in the same order of size. The comparatively low estimates of the groups of dots close together might be due to the fact that they were shown first and the observers had not become familiar with the experiment, and hence they could judge the later ones better because of the training they had had. To avoid being misled by this, I repeated the first three cards at the end of the series without the subjects knowing that they were the same cards they had seen at first.

It will be noticed that the records may be worked over in two different ways. Tables IV. and V. contain the results worked out according to distribution and according to size. The figures represent the sum of each observer's judgments for each class. In the last three columns, where size only is considered, the repeated series is added in with the rest. Each card shown three times makes 45 spots shown on each card, and three cards in a group makes 135 spots shown altogether in each group. When size alone counted, three of the cards were shown six times and the other six cards three times, so that the correct answer for the total would be 180. With *Ba* the experiment was repeated at a later date.

Table IV. Table V.

	Close.	Medium.	Scat.	Close Repeated.	Small.	Middle.	Large.
Mi	110	132	134	122	174	171	153
F	97	116	132		115	112	118
Ba	136	162	152	132	174	192	216
Ca	83	93	105	81	96	119	127
J	115	134	139	128	175	173	168
A	141	163	151	134	186	201	206
G	93	112	119	111	134	145	156
Ba	123	157	164	117	184	171	206
Ave.	112	133	137	117	154	160	168

Table IV. shows a gradual increase in number as the distance increases. There are but two exceptions: *Ba*, at his first sitting, and *A* report a greater number at the medium distance

than at the greatest distance. There are no exceptions in the first two columns. For all other observers there is an increase in each case, and necessarily the average increases. In the column containing the results of the repetition of the first group it can be seen that in no case is the figure as large as the corresponding figure in the second column. Although the average is a little larger than the average of the first column, it is much smaller than the average of the second column. By accident the group was not repeated for F.

Table V. shows a tendency to increase when the size increases, but the increase is not so marked as in Table IV., and six exceptions to the rule will be found. In Table IV. the increase of column two over column one is 18.8 per cent., and of column three over column two is 3 per cent. In Table V. the increase of column two over column one is 3.9 per cent. and of column three over column two is 5 per cent. This difference is what we should expect if we consider the chief factors which determine the judgment. Experience teaches us that a large space will hold more things than a small one, therefore if we have no other data than the amount of space occupied we conclude that the greater space contains the greater number. This is not always true, and when we have time enough we count and see, and even if we do not have time enough to count, we may make some allowance for the empty spaces between the objects, but the attention is directed chiefly to the objects and not to the spaces between them, and hence we are not likely to make sufficient allowance for the spaces. This seems to me to be the reason that the subjects think the groups most widely scattered contain the greatest numbers. The first thing that attracts attention when a card is shown is a group of spots occupying a certain amount of space, and the second thing is that these spots are separated from one another by certain distances. This second factor, being less prominent, has less influence on the perception, and hence it is not given sufficient importance in making the judgment.

When size is taken into account the case is somewhat different. The larger objects give the idea of moreness, but this is counterbalanced by the knowledge that into a given space there

can be put a greater number of small objects than of large ones. In neither case does the observer perceive the number, but he perceives a group possessing certain characteristics, and from these characteristics he infers the number. He probably never gives the same amount of attention to each characteristic, and, as one or the other is more prominent in his mind, his judgment is influenced in the corresponding direction. Concerning the method of inference, more will be said later.

Table VI., given below, illustrates very well one method of inferring number. Two cards, represented in the cuts as figures X and Y, were used. X was shown first, and the observer perceived the crosses, estimated their number and doubled it to get the number of lines. Y was then shown, and as it did not contain crosses there was no occasion for doubling. The amount of surface covered was exactly the same as in X. The natural conclusion is that there are not so many lines in figure Y as in figure X. There were five subjects. On three of them the experiment was made but once, and on one it was made twice on different days, and on the other, three times on different days. I give the results in full. The three columns represent the three successive judgments which each subject made.

TABLE VI.

J	X— Y—	30 12	30 15	30 15
T	X— Y—	48 30	48 30	48 36
A	X— Y—	30 15	30 20	29 25
M	X— Y—	40 20	40 20	40 20
M	X— Y—	40 20	40 20	40 20
Ba	X— Y—	18 12	24 12	24 12
Ba	X— Y—	24 16	24 16	24 16
Ba	X— Y—	24 12	24 12	24 16

The actual number of lines on each card is thirty. It will be noticed that some of the answers are correct and many are

incorrect. The accuracy of the answers matters but little, the point is to compare the judgments of X with those of Y. In general the records show that the number of lines in X is regarded as about twice the number that are in Y. This is a legitimate conclusion from the *prominent characteristics* of the two figures. If the observer had time to attend to *all* the characteristics of course he could judge better, but he cannot attend to all of them at once, and he must base his judgment on those prominent characteristics which he can observe. The experiment shows in part the method of inference, and it shows the necessity of perceiving prominent elements first and drawing conclusions from them.

In order to reduce the matter to very simple terms a little experiment was tried, which it is not necessary to report in detail. I mention the total result as obtained from eight subjects. Two cards were prepared, each having four lines on it, thus, (1) — — — — (2) | | | | . The lines were the same size and the distance covered by them the same in each case. They were first shown in the position indicated and then turned around so that what is here the horizontal direction became the vertical. Each card was shown, in all, sixty times in each position. The sum of the answers given for the two cards in the horizontal position was 493, in the vertical position 534. This may be related to the tendency to overestimate distance in the vertical direction. If the distance did seem greater it would be natural to estimate the number as more, according to the preceding experiments. The sum of the answers for card (1) in the two positions was 484, of card (2) in the two positions 543. The correct answer in each case would be 480. Each card was estimated as more in the vertical position. (1) was given as 230 horizontal and as 254 vertical. (2) was given as 263 horizontal and as 280 vertical.

It seems to me that here is clearly indicated one of the relations of number and space. Every one knows that a vertical column appears longer than a horizontal one of the same length. It seems also that when the vertical column is made up of separate units it appears to contain more of them. We must conclude one of two things: either the perception of num-

ber depends upon something which is analogous to the reasoning process, or the perception of number and the perception of space are conditioned by some common element. We might borrow a mathematical expression and say that one is a function of the other, and ignore the causal element, but such a statement is unsatisfactory because it fails to take account of all of the facts. We can say that one tends to vary as the other varies, but the tendency is sometimes overcome by other influences. Filled space tends to look larger than empty space, but even this is true only under certain conditions. Considering all the conditions and reservations it seems best to fall back on the hypothesis that the perception of number is analogous to the reasoning process. That process will be the main subject for consideration in the concluding remarks of this paper.

Plate II. affords an illustration of the effect of distribution on the perception of size. To most persons the upper circle appears larger than the lower. They are the same size and contain the same number of spots, but in one the spots are more scattered. To some persons the spots themselves look larger in the upper circle. For some observers the illusion quickly disappears, for others it continues much longer. When squares instead of circles are used the illusion is just as marked. It is more noticeable and more lasting if, instead of using lines to bound the spaces, two separate pieces of paper are cut out and spots arranged on them in the same way. One of these circles is filled with as many things as the other but it does not seem to be as *much* filled. One suggests expansion, the other contraction, but I see no reason for thinking there is any more eye-movement in one case than in the other, especially as the illusion is the more noticeable the quicker the glance. A careful comparison of the diameters of the circles causes it to disappear.

Conclusion.

The perception of time and the perception of space have been studied and discussed at great length, but the perception of number as such has received but little attention. Much has been written about the logic and function of the number series, anthropologists have sought to discover the origin of numerical

systems, the question of multiplicity in unity has been discussed by philosophers, and psychologists have tried to find out how many things we can perceive at once, but seldom has the question been asked: How do we know how many things are before us when we do perceive them at once?

I would be glad if we had a satisfactory term to apply to those apperceptive processes which when described seem like reasoning, but which when experienced are not like it. If it were not for the logical implications of the term I would call it immediate inference, *i. e.*, it is immediate in the sense that the middle term is not present to consciousness. Middle terms have been used in developing the concepts but are afterward forgotten. Perception itself might be described as a reasoning process in which the middle term is so completely forgotten that it cannot be directly discovered. For the want of any other I am compelled to use logical terms, but I do not at all mean to imply that the observer is aware of the elements which he himself uses in making his judgments. When I say that number is an inference, I mean that it is inferred in the same sense that the third dimension is inferred and apparently directly sensed. Psychologists have explained the process of perceiving depth, and when stated it much resembles reasoning, but the observer is not aware of the *quasi* reasoning process, nor can he discover it directly. No one can directly discover that the retinal image of a distant object is smaller than the image of a nearby object of the same size. Illusions of distance are common and we attribute them to wrong interpretations of sensation. Artists give the impression of depth by foreshortening certain lines. As the process of perceiving space has been studied and analyzed and the main elements of it described, so I have tried to study the process of perceiving number. If the size of the retinal image and other elements of experience lead us to think that one object is nearer to or farther than another, perhaps there are elements of experience which lead us to think that one group of objects is more numerous than another. The experiments described indicate what some of these elements are.

The most obvious feature is the similarity of the process of perceiving number and the process of perceiving space. Just

as space is an original element in consciousness, but spaces, *i. e.*, space relations, are the product of experience, so number may be called an original element, while numbers are derived. The original space perception must be one of purely indefinite space. It cannot be called the perception of an amount of space, because *amount* implies relation and comparison, and there is nothing with which to compare the originally given space. Whether the object be a speck of dust or a mountain, its spatial quality is the same, and hence it is impossible in any way to designate such a space. We have much the same difficulty with number. We have no difficulty in seeing what the first perception must be numerically, but we have become so accustomed to thinking of number as some particular part of the number series that when we say *one* we cannot think of it except in its relations in that series. The term unit is better in some respects, but we usually think of that as meaning the smallest part into which some quantity is divided. When I say that what is given at once is given as *one*, I do not mean one in the sense of a primary component of a larger group. I mean simply an undivided whole. This whole may be separated into parts and one part after another may become an object of consciousness, and by setting these parts into correspondence with a previously constructed number series we may enable ourselves to remember in the future into how many parts we have found it convenient to separate this whole. Thus it becomes possible to assign the proper number to a given group without stopping to count, which means regarding one thing after another. This does not mean that we perceive the number of things in a group. It means that we perceive the group as a unit and remember that one of its characteristics is that it can be separated with advantage into a certain number of parts. One can immediately recognize a cube as a six-sided figure, but he does not perceive the six sides at once. There is no constant objective standard of unity. Sometimes one thing and sometimes another determines the point at which analysis shall stop. Continuity in space affords, perhaps, the most common basis of unity, but it is not enough in itself. The necessities of description often determine the limit of analysis.

No one could regard a cube as having seven sides instead of six, but that is because he has already limited himself by defining *side*. If one is asked how many parts there are to a cube he may as well say eighteen — counting surfaces and edges — and he need not stop here, he may as well include the angles, and so on as far as the acuteness of his own intellect will permit him to go. How far the analysis might be carried in any particular case matters not, the process is the essential thing. The process in brief is this: (1) The perception of an undivided whole, (2) the analysis of that whole into parts — this involves temporal succession, (3) setting the parts into a correspondence with the number series for the convenience of description and fixation by association. When this has been done enough times to firmly establish the association the process is no longer necessary, and the observer may come to think he directly perceives the number. When a group is presented to him he at once associates that impression with the number which he has assigned to it before. This act is nothing more than naming it. The only difference is that names are usually without connotation, while number connotes certain qualities. If names carry with them some significance, then there is no difference whatever. Both the name and the number connote qualities which need not be recognized or even remembered at the moment. One may recognize a horse as a quadruped without at the same time thinking that he is four-footed. One may recognize four lines as four without thinking of them as one and one and one and one. If he is asked how he knows it is four, he must say because he remembers that that unit is of the sort that he has separated into four parts and he has habitually regarded it as four. Four is a name for a certain object of sensation, and for sensation it is a unit, but a unit of such a character that it can be separated into parts and each part in turn become an object of sensation. If this separation does not take place the object remains a unit no matter by what number it is designated.

When figure G was given it was usually recognized as a thing of a certain form and that form is known by experience to consist of a certain number of parts. Sometimes it was necessary to make a drawing of it in order to count the parts (see page

30). By this method the observers were often able to give the correct answer. Evidently they had a picture of the *thing* in mind before beginning to draw, but that picture was not made up of parts. It was a unit which could be divided into parts. So far as a description of the object is concerned these two expressions amount to the same thing, but as a description of mental content they are altogether different. We see the same procedure exemplified in all the experiments. That which represents a thing, *i. e.*, has a known form, is associated with the number which describes its parts. Figure C was easy to recognize because of the familiarity of its form. Figure D conveyed less definite suggestions, and hence was not perceived so accurately. Figure B was very simple in form but less familiar than C, and as familiarity rather than simplicity determines the ease of judgment[1] we should expect it to be seen less accurately. Figure F was difficult because it suggested a form which it did not possess.

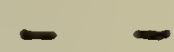
If the observer perceived the lines as lines, there is no reason why he should not perceive five as easily as eight, and he should do so more easily, but if he perceives a *thing* and associates some idea with it, then the more frequent the association the more easily is it made. This explanation at first sight may not seem to account for the perception of a group of irregular dots which do not form a familiar thing at all. They do not possess a form which can be named, and it may not be exactly like anything that has been seen before, but the group nevertheless does possess unity, form and a certain amount of familiarity. It is a little bunch of objects occupying a certain amount of space, it is more or less compact or diffused and hence has a quantitative character. We have, in a general way at least, associated number and quantity, and the judgments of number were based on just these elements when no others were present, or when the others were less prominent. A comparison of figures X and Y shows an extreme case of dependence upon the space-quantity relation. This must be depended upon when better means are not afforded, or when other factors are less prominent. In the experiments on touch it was found that

[1] See *Harvard Studies*, p. 138.

the subjects always mentioned the prominent points first and then looked for the others. This could be done in the touch experiments because the stimulation was of longer duration. In the experiments on sight the stimulus was removed so quickly that there was no time to look for factors not seen at first.

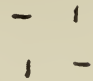
In the comparison of the regular and irregular figures it was seen that the form was the *prominent characteristic* in the regular figures. That is, we depend chiefly on form whenever it is definite enough to be recognized. We saw too that, although we are less likely to make errors when the figure is regular, if we do make them they are likely to be greater, because it is not possible to add or subtract just a little from a given form without thereby changing the form, hence when an error is made it must be great enough to preserve the form providing the form is recognized. Sometimes an observer would mistake one form for another. A figure whose vertical length was greater than the horizontal might be drawn with the horizontal length greater, and *vice versa*. This may have been due to poor memory. The actual form might be recognized but its position forgotten. The error is similar to the common error in touch in which the observer recognizes the number of points but locates them in the wrong direction.

Enough has perhaps been said about the method of analyzing objects of sensation and of assigning numbers to them by association. There remains but to mention the reason we analyze things according to their space relations so much more often and more closely than we do according to their qualitative differences. The 'vital question' with every one is not *what* things are, but *where* they are. Even the scientist, who, more than any one else, is interested in the *what* of things, spends the most of his time studying their space relations. The microscope is of service for this only. Eye-glasses prevent misconceptions of space relations. The person who is color-blind loses but little, but if a person were unable to perceive space relations he would be unable to live, merely because he could not find out *where to get* things.



B

C



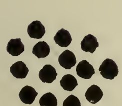
D

F

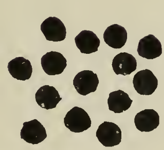
G

X

Y



O (1)



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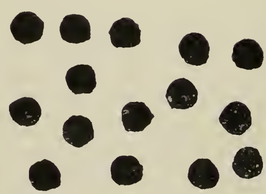

O (9)